

## CORRESPONDENCE

MADAM,—In their recent papers on the Gilbert Murray experiments, Dr Dingwall and Professor Dodds consider the hypothesis that Murray's successes may be attributed to unconscious auditory hyperaesthesia. Professor Dodds rejects the hypothesis but his remarks apply mainly to *conscious* hyperaesthesia (Dodds, 1972, pp. 398-401). Dr Dingwall, offering anecdotal evidence for auditory hyperaesthesia, argues that it may well have been the cause of Murray's successes, but his remarks too would seem to apply only to conscious hyperaesthesia (Dingwall, 1973 pp. 22-24 and pp. 37-38).

I should like to make three tentative observations: (1) it is doubtful that conscious hyperaesthesia exists; (2) although unconscious hyperaesthesia is a genuine phenomenon, it was probably not the cause of Murray's successes because it does not lead to direct knowledge of what is 'unconsciously perceived'; and (3) an explanation in non-paranormal terms is, nevertheless, possible. May I deal with each in turn?

(1) In the psychological literature hyperaesthesia is usually discussed in connexion with hypnosis. The topic occupies a page or so in the chapter on hypnotism in William James's *The Principles of Psychology* (James, 1890). James evidently believed hyperaesthesia to be a common symptom of hypnosis. He provides examples of the enhanced sense of touch of a hypnotized person, and notes, all too briefly, that 'auditory hyperaesthesia may enable a subject to hear a watch tick, or his operator speak, in a distant room' (op. cit. p. 609). His account of visual hyperaesthesia is fuller and is interesting reading. One demonstration he refers to as 'the ordinary test of visual hyperacuteness in hypnotism'. The subject is given the hallucination of a picture on a blank card that is presented to him. The card is then shuffled with a pack of similar cards but the subject can always find it again. The subject notices peculiarities on the card that are, James would have us believe, too small for normal waking perception to detect. James does add though, in a cautious footnote, that one's ordinary ability to discriminate blank cards and sheets of paper from each other is much greater than one would suppose.

The subject in McDougall's five stamps experiment showed a comparable sensitivity to seemingly imperceptible features. Instead of blank cards five mint postage stamps of the same denomination were used. A negative hallucination for two of the

stamps was induced, and the hypnotized subject persistently behaved as if he did not see them even when their positions had been changed whilst hidden from view. He must, however, have seen these two stamps and/or the others in considerable detail (McDougall, 1926). McDougall warns us:

...not to suppose that the discrimination ... implies some extraordinarily increased perceptual acuity; for any normal person can, by close inspection, discriminate and recognise one or two postage stamps among others. The books contain many statements about marvellously increased powers of perception on the part of subjects in hypnosis. To the best of my belief these are in the main errors, founded largely on the reporters' ignorance of the fineness of our discriminations. (Op. cit. p. 93.)

More recently, McKellar and Tonn attempted a repeat of McDougall's original experiment (McKellar, 1968). The subjects were two male American university students. Negative hallucinations for two of five stamps irrespective of position were induced only after much practice with easily discriminated objects, like chess men and stamps with the margin paper left on them. As a control, six non-hypnotized subjects, five male and one female, were given the final task of discriminating two from five similar stamps. They were successful in only two out of twenty-three re-presentations. It could be argued, however, that this result was due to the demand characteristics of the experiment or to experimenter effects (Orne, 1962; Rosenthal, 1963). One subject refused to believe the task was possible and, therefore, did not try. Four subjects felt they could probably have been able to discriminate the stamps but felt they lacked 'effort' or 'psychic energy'. One subject was one of the two hypnotized subjects in the first part of the experiment, and he apparently wished he could have put as much concentrative power into the task as he had done while hypnotized (McKellar, 1968 p. 67). All this suggests the subjects knew the purpose of the experiment and may unwittingly have produced a result desired by the experimenters. In support of McDougall, Marcuse reports an experiment in which subjects were able to discriminate stamps even when their perforations and margins were concealed by a special frame (Marcuse, 1959 p. 99). The experiment is a simple one to repeat, but it is possible that the results will depend on the quality of the paper used in the printing of the stamps!

In a comprehensive review of experimental work on hypnosis (Hull, 1933) only one properly controlled investigation of hyperaesthesia could be reported. P. C. Young, a pupil of McDougall, investigated cutaneous sensitivity to faint pressure and found

that hypnotic suggestion, if anything, decreased sensitivity (Young, 1925; Hull, 1933 pp. 245-248). Nevertheless, all twelve of Young's subjects believed they possessed 'greatly augmented powers of perception'. Hull remarks that 'this observation indicates one of the major sources of the long-prevailing belief in the power of hypnotic suggestion to induce gross hypersensitivity.' (Op. cit. p. 248.)

Unusual powers of hearing are sometimes discussed in books and articles on the psychology of music and on the hearing of the blind. These powers are not abnormal or pathological; they can be explained in terms of practice and increased attention. (One exception may be 'absolute pitch'. See Shuter, 1968 pp. 162-164.)

Much work has been done by physiologists and psychologists on the limits of hearing and on speech perception, yet there is nothing on abnormally acute hearing in standard accounts of these topics. (See, for example, Miller, 1951; Stevens & Davis, 1938; Thurlow, 1972; Woodworth & Schlosberg, 1954.) One reason for this omission may be the incredible sensitivity and versatility of *ordinary* ears.

(2) Presumably, 'unconscious hyperaesthesia' is the same as what is usually referred to, in the psychological literature, as 'subliminal perception', a term equally confusing. Dixon has suggested 'subliminal reception' would be more appropriate, although in his book he retains the popular term (Dixon, 1971). In subliminal perception, the percipient (or recipient) responds to a stimulus that is below his threshold, or limen. He is, of course, unaware of the stimulus, and, therefore, subliminal perception is a sub-species of 'unconscious perception'. Other kinds of unconscious perception are involved in phenomena such as, for example, an external stimulus determining the content of a dream, psychogenic blindness, and, perhaps, hypnotically induced negative hallucinations. They are distinguished from subliminal perception because in each case the stimulus is above normal threshold. (See Dixon, op. cit. pp. 12-13.)

I think I am right in saying that subliminal perception would not enable a percipient to know what the stimulus he 'perceives' actually is. For example, if a sentence, or phrase, is 'heard' subliminally the hearer will not know what the actual words are nor will he be able to say what the meaning of the sentence, or phrase, is. His subsequent behaviour may be influenced but he will not know why except by inference.

However, the subliminal perception of words may influence subsequent behaviour, including verbal behaviour, in such a way that it would be reasonable to suppose that somehow the per-

cipient does apprehend the meaning of the words. An example will make this clearer. Dixon conducted an experiment in which subjects had to write down the first word that came to mind every time a visual signal was given. The signal was preceded on alternate presentations by the subliminal auditory presentation of a word drawn from a list of words classified according to emotional connotation. The experiment, in fact, could be described as a subliminal-word association test. That the subjects reacted to the meaning of the emotional words at least was inferred from the longer response latencies of items (i.e. from the fact that it took the subjects a longer time to think of a word after the subliminal presentation of an emotional rather than a neutral word). Furthermore, the responses themselves were often related to the subliminal words and many were Freudian associations (Dixon, 1956; Dixon, 1971 p. 71 et seq.).

It seems to me that there were too many 'direct hits' in the Gilbert Murray experiments to suggest an explanation in terms of subliminal hearing. On this hypothesis one would not expect responses to correspond so closely to the literal meaning of the subliminal stimuli, yet 54 of Murray's 128 responses in Series C, for example, were 'complete successes' and 36 were 'partial successes' (Dodds, 1972, p. 394, Table I).

(3) If the conditions of the experiments were such that Murray may have subliminally perceived the words spoken by the principal agent then it is possible that he could also have perceived some words, or parts of them, *supraliminally* (i.e. heard them in the ordinary way). The sound intensity level of the principal agent's voice may have been fluctuating and at some point the words may have been just loud enough to have been heard. Even without variation in intensity, some words would have been heard *supraliminally* at a level that permitted subliminal hearing of most of the others. (This would not have been the case if most words had been well below threshold and only one or two perceived subliminally.) Acute, but ordinary, hearing, then, seems to me a plausible hypothesis.

I am not implying that Murray cheated. Certainly, it is reasonable to accept that had Murray actually heard what was uttered, and been aware of the fact, he would have said so. Why, then, did he not report hearing the words spoken by the principal agent? One possibility is that Murray *did not know* he was hearing, strange that this may seem. He may have confused perceptual with imagery experience.

A confusion of this type was first demonstrated by Perky. Her subjects were asked to project visual *mental* images on to a

screen positioned on the wall of the experimental room while, without their knowledge, they were actually seeing faint *physical* images produced by a projection apparatus behind the screen and in an adjoining room. The screen was in effect part of the wall common to both rooms. The subjects believed they were imaging but it was clear that their experiences were determined, in part at least, by the physical images. The experiment showed that there is no sharp division between imagery and perception (Perky, 1910). The Perky effect has been demonstrated in more recent experiments, and has been found to depend on certain personality characteristics (Segal & Nathan, 1964; Segal & Glicksman, 1967; Segal & Gordon, 1969; see also Richardson, 1969). It has also been shown that imagery can significantly reduce sensitivity to visual stimuli (Segal & Fusella, 1969).

If these findings apply also to auditory imagery and perception (I do not know of any published work on the Perky effect for the auditory sense but can see no reason why it should not occur), then what happened in the Gilbert Murray experiments may have been as follows:

(i) Murray prepared himself to receive an 'impression', and thereby triggered off his brain mechanism for imaging.

(ii) He heard, just above threshold, a crucial word or two, or parts of words, but what he actually heard he mistook for an auditory image. These words would probably have been from the initial part of the principal agent's utterance.

(iii) The word, or words, then became part of the content of Murray's thought as an auditory image, or perhaps a verbal-motor image, and may also have aroused imagery in other modes.

(iv) The imagery reduced the sensitivity of his hearing, so the rest of the utterance was not heard. (It may have been heard subliminally.)

(v) Murray then inferred the details of the scene or incident contemplated by the principal agent from the crucial word, or words, and his own knowledge of the scene or incident.

Only one or two words, provided they were crucial to the meaning of the principal agent's utterance, need have been heard for Murray to have achieved a 'hit'. The rest could have come from inference. This applies also to subliminal hearing, should crucial words have been obtained by it, and, of course, to telepathy. Indeed, the scenes and incidents thought up by the principal agent should have been chosen randomly. (See Carington, 1940 pp. 45-46 and 1946 pp. 10-11.)

In conclusion, I must emphasize that the explanation I offer is merely an alternative, one of seven: telepathy, subliminal hearing,

supraliminal hearing confused with imagery, a combination of two or a combination of all three of these processes. We shall never really know what occurred.

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MADAM,—I am glad to see that the papers on Murray by Professor Dodds and myself have occasioned several interesting and suggestive letters and those by Mr Hilton, Mr Farge and Professor Rushton are specially to be welcomed. Although Mr Farge's contribution seems to me somewhat obscure and the importance he attaches to a number of papers by certain psychologists is, in my opinion, perhaps not wholly justified, where I fully agree with him is that we shall never know what really occurred and thus I shall not indulge in further speculation. What is now required is further carefully designed experiments by persons competent to deal with the many difficulties which abound in this complex field. What I think is perhaps the most curious feature of the whole correspondence is the apparent unwillingness of any of the writers to attempt to solve the enigma presented by Murray himself and the contradictions between his statements and his acts, and also the extraordinary methods employed by the experimenters and their apparent refusal to settle the question between telepathy and hyperaesthesia by instituting control conditions of the simplest character.

A few words in conclusion. On pp. 34-35 of my paper I discussed briefly the dream of the incident of the Turkish officer mentioned by Professor Dodds (p. 400) and which he thought might suggest ostensible telepathic powers. One very odd feature of the dream was the mention of the Campbell clan and I said (p. 35) that more light might be thrown on the incident if the Campbell association could be traced. I tried in vain to discover

any association of this sort but eventually I was put in touch with a lady who thought that what she had to tell me was relevant. I am very grateful for her interest and patience in view of my many questions and requests for details. She was the daughter of a Commander Campbell and the whole family were close friends of the Murrays at Boars Hill. In 1921 a friend of Commander Campbell was stationed at Constantinople, where he spent six out of every twelve months. Since the Treaty of Lausanne between the Entente powers and Turkey was in 1923, Commander Campbell's friend may very easily have heard the story of the dinner in the restaurant and told Campbell, who may have repeated it when at Boars Hill in the course of ordinary conversation. This might be the connection between the dream and the name Campbell, since in repeating the story Murray might have thought that the incident had happened to Campbell himself and not to someone connected with his friend in Constantinople. Whether the fact that the dream occurred the night after Mrs Toynbee thought of telling the story to Murray was coincidental or not we do not know, since the full facts regarding the affair can now never be ascertained.

ERIC J. DINGWALL

MADAM,—I was greatly interested in Mr Richard Sheargold's letter in the *Journal* for December, 1972, p. 225, especially after reading extracts from other letters of his given in Peter Bander's *Carry on Talking: How Dead are the Voices?* (Colin Smythe, Gerrards Cross, Bucks, 1972). In these earlier letters Mr Sheargold speculated first on 'some obscure form of incipient mediumship' but later testified to having heard a woman's voice which seemed to him genuine judging by 'the speed of utterance and the unmistakable rhythm'. In the letter in the *Journal*, Mr Sheargold admits that he is now satisfied that 'the phenomenon is a reality quite apart from the testimony of experts' though the problem of the origin of the voices remains.

I am trying to get my niece's husband in Boston, an expert in electronics, interested in the problem, with what success I do not know yet. Meanwhile I should like to refer to an alleged phenomenon in India which raises problems of nearly the same order as the 'Raudive voices'. In an article in the *Journal* for September, 1973, I gave a brief account of a contemporary South Indian Saint, Sri Sathya Sai Baba, who is asserted to produce the most prodigious and incredible range of parapsychological phenomena. The psychical horizons of India are, of course, unbounded. Three